PHYSIOLOGY.

5. *Experiments on Pulmonary Absorption and Exhalation.*—M. Collard de Martigny has published in the *Journal Complementaire* for May and August last, an account of some interesting set of experiments, undertaken for the purpose of determining some of the disputed points in the chemical physiology of respiration. Not having received that Journal, we are indebted to the *Edinburgh Medical and Surgical Journal*, for a summary of his principal results. They are the following:—

1. According to the doctrine of Lagrange, which is a modification of the original theory of Lavoisier, oxygen gas is absorbed *in substance* by the venous blood in passing through the lungs, and does not unite with oxygen to form carbonic acid till it has proceeded with the arterial blood to the capillaries. This opinion rests merely on some imperfect experiments by Girtanner, who thought he discovered oxygen in arterial blood. It is singular that these experiments should not have been repeated till now, as they obviously, if correct, lead to a very precise and important conclusion. On trying them, M. Collard de Martigny procured results decidedly negative. Having filled a tube thirty-six inches long with mercury, and reversed it so as to produce a barometric vacuum, he admitted about an inch of fresh arterial blood from the crural artery of a living dog, and left the apparatus at rest for an hour and a half. At this time, the mercury having descended considerably, the gas which had been developed was transferred into a graduated tube, and acted on by caustic potass. The whole of it was entirely absorbed, and consequently consisted of carbonic acid only.

2. The more prevalent doctrine at present is, that the oxygen which is absorbed by the blood in its passage through the lungs *unites with* the blood; that carbonic acid is formed in the capillaries by the various processes of nutrition and secretion; and that the carbonic acid thus formed is given off in the lungs by a process of exhalation and secretion, independently of the presence of oxygen. This doctrine rests mainly on an experiment first performed by Girtanner and afterwards more carefully by Edwards—where a frog, being made to breathe hydrogen alone, gave off nevertheless more than its own bulk of carbonic acid in the course of a few hours. This result, if the mode of experimenting is free from fallacy, is decisive of the question. It proves, that the carbonic acid given off during respiration, is not formed in the lungs by the union of oxygen with the carbon of the venous blood in its passage through the pulmonary circulation, but arrives with the venous blood in the lungs ready-formed, and is, in short, the product of the various functions of the capillaries. But M. Collard de Martigny considers, that even the method of experimenting pursued by Dr. Edwards its liable to fallacy. On the one hand, hydrogen and carbonic acid are so different in density that they mingle slowly, and in consequence an analysis of a portion of the mixture does not represent the composition of the whole mass. And on the other hand, according to a law in physics, the tissues of the body, being impregnated with carbonic acid, must give out that gas when immersed in an atmosphere of any other gas. To obviate the former fallacy he substituted azote for hydrogen; and to do away as much as possible with the latter, he withdrew and analyzed the air in which the animal was confined once every hour or every two hours and replaced it by pure azote—so that in this way he could ascertain whether carbonic acid was given out only at first, and therefore arose merely from the displacement of the gas diffused through the textures, or continued throughout the whole duration of the experiment, and consequently arose from the secretion of the gas by the lungs. In every experiment he found carbonic acid given out in considerable quantity as Edwards also invariably remarked. In an interval varying from seven and a half to nine hours, he procured in seven different experiments from $2\frac{1}{2}$ to $2\frac{3}{4}$ centilitres, or between $1\frac{2}{3}$ and $1\frac{3}{4}$ cubic inch of carbonic acid. In every instance

he found nearly twice as much gas formed during the first period, as during any subsequent period, which he attributes to the displacement of carbonic acid in the textures by the azote. But after the first period the quantity formed in every equal period of time was nearly the same till the animal began to become torpid and the respiration to languish, when the formation of carbonic acid rapidly decreased. Hence he concludes that after the first period, the carbonic acid is derived solely from the blood in the lungs.

The exhalation of carbonic acid from the lungs, then, is independent of oxygen being supplied to it. The presumption must consequently be, that it is secreted or excreted by the blood in the lungs. But in order to establish this doctrine satisfactorily, it is necessary to show that the blood in passing through the lungs, really loses carbonic acid—a point which no one before M. Collard de Martigny has endeavoured to ascertain. He has proved it, however, as he conceives, by a comparative examination of the arterial and venous blood of the same animal. When each was collected directly from the blood-vessels in a barometric vacuum, as formerly described, he found that venous blood almost always gave out twice, and on one occasion, thrice as much carbonic acid as the arterial blood. But when respiration was suspended by exhausting the lungs, and tying the trachea, the arterial blood was found to contain as much carbonic acid as the blood in the veins.

5. Physiologists have differed with one another as to the question, whether the carbonic acid gas given out in the lungs is equivalent to the oxygen absorbed by them; but, on the whole, the prevailing opinion is, that more oxygen is absorbed than is accounted for by the carbonic acid exhaled. M. Collard de Martigny arrives at the same conclusion by a series of experiments, apparently more free from fallacy than any previously made. The chief difficulty is to avoid the fallacy arising from the probability of the air in the lungs of the animal at the beginning of the experiment not corresponding in quantity with what remains at the end. The author got rid, as he conceives, of this difficulty, by not confining the animal in the jar of air to be breathed, but by fixing a tube in its trachea, exhausting the air in its lungs, then establishing a communication by means of the tube between the lungs and the jar, and, at the end of the experiment, expelling the residual air of the lungs into the jar by strong pressure of the chest. He has related the particulars of nine experiments of this kind, of which eight were performed with the rabbit, and one with the Guinea-pig. In four of them, the quantity of air breathed was four *litres*, or 244 English cubic inches; in four others, it was three *litres* and a half, or 213 cubic inches, in one it was 183 cubic inches; and the duration of the experiment varied from nine to fifteen minutes. In the largest quantity of air, the quantity of oxygen was 51.2 cubic inches. Of this there remained unaccounted for by the residue of oxygen, together with the carbonic acid evolved, 6.5, 16.3, 16.9, and 16.9 cubic inches in four different experiments. In the four experiments with 213 cubic inches of air, the total oxygen being 44.8 cubic inches, there remained unaccounted for, in like manner, 2.3, 4.7, 7.1, 10.4, and 18.4 cubic inches. In the experiment with 183 cubic inches, where the oxygen was 38.4, the quantity unaccounted for was 4.7 cubic inches.* In all these experiments, then, it is clear, that a large, though variable quantity of oxygen gas disappears—a larger quantity is absorbed than is given off in the form of

* The particulars are contained in the following tables, which are carefully calculated from the original tables of the author.

Oxygen in Air before Respiration.	Oxygen remaining.	Carbonic Acid.	Oxygen disappeared.
1. 51.2	23	12.3	16.9
2.	27.1	17.6	6.5
3.	22.6	11.8	16.9
4.	20.3	14.6	16.3
5. 44.8	17.4	17.0	10.4
6.	14.4	12.0	18.4
7.	19.7	22.7	2.3
8.	18.7	19.0	7.1
9. 38.4	15.3	18.4	4.7

carbonic acid. But we must observe that M. Collard de Martigny commits a serious oversight in supposing that his method of experimenting is free of fallacy, or represents natural respiration. On the contrary, after the first inspiration, the animal breathes an atmosphere considerably impregnated with carbonic acid gas; consequently the blood absorbs the gas, which it is very well known to do when a moderate proportion of carbonic acid in the air is presented to it; and in this way the apparent disappearance of oxygen may be sufficiently accounted for.

4. Another point of dispute among physiologists who have occupied themselves with this subject, is whether any azote is given off or absorbed. On the whole, the greater number of authorities unite in finding that a small quantity is given off. But of late, much confidence has been reposed in the experiments of Dr. Edwards, who found that azote is sometimes absorbed, and sometimes given off, according to the season of the year. M. Collard de Martigny is at variance, however, with Dr. Edwards on this point, having invariably found in many trials at different seasons that a small quantity of azote is given off. In the experiments formerly mentioned to determine the proportion of oxygen which disappears in respiration, he found in four trials with 244 cubic inches of air, that azote was exhaled to the amount of 1.9, 1.8, 1.6, and 0.1 cubic inch; in four trials with 213 cubic inches, the quantity exhaled was 4.1, 3.6, 1.8, and 1.3 cubic inches; and in one trial with 183 cubic inches, the quantity was 4 cubic inches.

5. Lastly, M. Collard de Martigny considers the question, whether any water is formed in the lungs by the union of the oxygen of the air with the hydrogen of the blood. The idea that water is so formed has been, we believe, universally abandoned in Britain for some time, and we were not aware that this branch of the Lavoisierian doctrine of respiration still met with its favourers in France. It may be at the same time true, as our author states, that it is a notion more easily rejected than disproved. The objection first urged against it, that hydrogen never unites with oxygen at so low a temperature as 100°, was met with the rejoinder, that such union readily occurs when the hydrogen is in a *nascent* state. But M. Collard de Martigny objects, that according to his own experiments, and those of M. Chevallier, hydrogen is never in a nascent state brought in contact with azote without ammonia being formed, which he has never found in the halitus of the respiration. Another objection is, that whether common air or azote be respired, the quantity of halitus formed is pretty nearly the same.

In a paper which will appear presently, the author of the present essay undertakes to prove that *animal heat is altogether independent of respiration.*

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6. *Animal Heat.*—It has been maintained by many physiologists, that animal heat was entirely generated in the lungs by the process of respiration; and they attempt to account for its increase in those diseases in which the lungs are rendered impervious to air; by the supposition that respiration is in such instances carried on with more rapidity in the parts still pervious. A case entirely at variance with this hypothesis is related by Drs. Graves and Stokes. The patient laboured under very extensive development of tubercles, had tubercular abscesses in the superior portions of both lungs, and general bronchitis. In this case, at a period when the skin was hotter than usual, and the pulse 126, the respirations were only 14 in the minute.—*Dublin Hospital Reports, Vol. V.*

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7. *Circulation in Vegetables.*—On the 27th of September, MM. Henri Cassini and Mirbel made a report upon the vegeto-anatomical and physiological observations presented by Dr. Schultz to the Academy of Sciences. It appears that a circulation takes place in vegetables, comparable, in some respects, to that in animals. In fact, when the vessels in a portion of stem, an inch or two long, and two or three lines in width, are considered, assent cannot be refused to the idea, that a vital juice exists, and that it passes several times by the same

path. But there is this remarkable difference between the circulation in vegetables and in animals of a high order, that in the latter there is one point in which terminate two vascular systems very distinct from each other, one carrying the blood to the extremities of the body, the other collecting it and conducting it to its source; nor any double vascular system. Vessels of the same nature form a net-work, of which the meshes are so many similar circulating apparatus communicating with each other, so that there is a common motion through them whilst the parts live together, and a motion proper to each so soon as they are separated. The discovery of M. Schultz is of the highest interest for the anatomy and physiology of vegetables; it enlightens these two branches of science, the one by the other, and it proves relations to exist between animals and vegetables, which before were not even suspected to exist.—*Journal of the Royal Inst. G. B. Feb.* 1831, *from the Ann. de Sciences Nat. Vol. XXI.*

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8. *Mutual action of Blood and Atmospheric Air.*—Professor Christison, one of the most learned medico-legal jurists and skilful chemists of the present day, has communicated to the Royal Society of Edinburg, some extremely interesting experiments instituted for the purpose of ascertaining what changes really take place in the blood when exposed to the action of atmospheric air, and whether the arterialization of the blood in the lungs is a vital or physical process.

It has generally been considered by physiologists and chemists, that when venous blood is brought into contact with atmospheric air out of the body, the blood changes its colour from dark purple to bright crimson, while the air loses a part of its oxygen and acquires carbonic acid. This has, however, been lately denied by Dr. John Davy, who states as the results of many trials, that atmospheric air and blood recently drawn from a vein have no mutual action whatever; that the colour of the blood is not changed; that no oxygen disappears from the air, and that no carbonic acid is formed in it. With regard to the change of colour in the blood, Dr. Christison says that in his experiments, purple venous blood always became, when agitated with air, brightly crimson, and the difference of tint was so great that no one could mistake the two varieties of blood.

Dr. Christison is also at variance with Dr. Davy, as to the alterations which the air undergoes during the changes effected in the colour of the blood, and his experiments appear to us conclusive. These experiments prove that when venous blood acquires the arterial colour by agitation with atmospheric air, that a considerable portion of the oxygen of the air disappears, that carbonic acid is formed, and that the process of arterialization, so far as regards the changes which the blood undergoes in colour, and the air in composition, is a chemical and not a vital phenomenon.—*Ed. Med. and Surg. Journ. Jan.* 1831.

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9. *Seat of the Sense of Taste.*—The following general experiments and conclusions are from a work on the seat of this sense, by MM. Guyot and Admyrauld. I. If the anterior extremity of the tongue be enclosed in a very soft, flexible case of parchment, so as to cover it completely, jelly, and in general all bodies may be introduced into the mouth, and crushed between the teeth without any taste being distinguishable. The same effect is obtained also by retaining the tongue apart from the cheeks or teeth; sapid objects placed beyond its action give no sensation of taste. The tongue, therefore, is the essential organ of taste; the lips, palate, cheeks, and gums have no power of this kind.

II. Nevertheless, if the tongue be entirely covered, and very sapid substances be swallowed, a little taste is perceived at the posterior part of the *velum palatum*. If the palatal arch be covered with parchment, a sapid body produces its ordinary effect upon the tongue. If a little piece of extract of aloes be fixed upon the end of a rod, and passed over the palate and the roof of the

mouth, it produces no other sensation than that of touching; but on the anterior and upper part of the soft palate there is a small portion of surface, not having definite limits, where the impression of sapid bodies is very sensible; the back part of the mouth does not partake in this property, so that this small portion of the palatal vault with the tongue forms the organ of taste.

III. If the tongue be covered with parchment, pierced at the middle of its back surface, sapid bodies applied to the part produce no taste, until, being dissolved in the saliva, they gain access to the edge of the tongue. Extract of aloes passed over various parts of the tongue produce sapid impressions within a space of only one or two lines at the sides, three or four at the point, and within a curved space at the back. Hence this part of the tongue and the lateral portions are the especial organs of taste in deglutition; and to the portion of the soft palate already mentioned prolongs the sensation.—*Journal of the Royal Inst. of G. B. No. 2, from the Bib. Univer.* 1830.

10. *Brown's Moving Molecules.*—We gave in Vol. IV. p. 200, of this Journal, an account of the discovery of active molecules in inorganic bodies, by Mr. Brown, and at page 474 of the same volume, and p. 475, Vol. V. will be found some further illustrations of this curious phenomenon. M. Muncke, of Heidelberg, has also been investigating this subject, and we find the following notice of his experiments in the *Journal of the Royal Institution of Great Britain* for February last.—M. M. finds the following a simple and easy mode of showing the motions of particles;—triturate a piece of gamboge the size of a pin's head in a large drop of water on a glass plate; take as much of this solution as will hang on the head of a pin, dilute it again with a drop of water, and then bring under the microscope as much as amounts to half a millet-seed;—there are then observable in the fluid small brownish-yellow points, generally round, (but also of other forms,) of the size of a small grain of gunpowder, distant from one another from 0.20 to 1 line. These points are in perpetual motion, varying in velocity, so that they move through an apparent space of 1 line in from 0.5 to 2 or 4 seconds. If fine oil of almonds be employed in place of water, no motion of the particles takes place, but in spirit of wine it is so rapid as scarcely to be followed by the eye. This motion certainly bears some resemblance to that observed in infusory animals, but the latter show more of voluntary action. The idea of vitality is quite out of the question. On the contrary, the motions may be viewed as of a mechanical nature, caused by the unequal temperature of the strongly illuminated water, its evaporation, currents of air, heated currents, &c. If the diameter of a drop be 0.5 of a line, we obtain, by magnifying 500 times, an apparent mass of water, of more than a foot and a half broad, with small particles swimming in it; and if we consider their motions magnified to an equal degree, the phenomenon ceases to be wonderful, without, however, losing any thing of its interest.

PATHOLOGY.

11. *Cause of Stammering.*—In our sixth volume, p. 233, et seq. we published Dr. Arnott's explanation of the nature of stammering; and we find in a recent No. of the *Journal of the Royal Institution of Great Britain* some interesting observations on this explanation, by Marshall Hall, M. D. which we will now lay before our readers.

Dr. Hall is of opinion that Dr. Arnott's view of the subject is so far from being correct, that it is quite plain that it is only in the articulation of certain letters that expiration is interrupted, and even in this case the interruption is not in the larynx, the organ of voice, but in some part of the mouth, or organ of speech. "It will assist us," says Dr. Hall, "in the determination of the question, to take a review of the influence which the natural articulation has